AIR FORCES ON CIRCULAR CYLINDERS,

Axes Normal to the Wind,

With Special Reference to the Law of Dynamical

Similarity.

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I. INTRODUCTORY

One of the pressing problems of theoretical as well as practical interest in present day aerodynamics is the proper means of passing from results obtained on small models in a wind tunnel to results valid for the full scale body. Much work has been done of an empirical nature on aerofoils and forms directly suited for practical use, but so far no extensive investigation has been made of air forces on a body of simple geometrical form over a large range, so that the suggested laws might be tested. The object of the present investigation was to test the validity of the law of dimensional similarity, proposed a long time ago by Lord Rayleigh[1], over a wider range than has heretofore been done, using as models circular cylinders with their axes normal to the wind.

Lord Rayleigh showed that under the assumptions usually made the force of a current of air upon a solid body may be expressed as $C\rho SV^2$, ρ being the density of the air, S the area of the solid projected on a plane normal to the wind, V the velocity of the wind, and C a dimensionless constant depending on a single parameter $\frac{VL}{\nu}$, where ν is the kinematic viscosity of the air and L is a linear dimension of the solid. Certain features of the results made it advisable to make in addition some measurements of the pressure distribution. Altho the investigation is by no means completed, the results

are so contrary to current views that it is felt advisable to bring them to the attention of other wind tunnel experimenters.

The previous work on wires and cylinders (axes normal to the wind) is very scant. In fact but three investigations in any way complete have been found; namely those of Föppl[2] at Göttingen, those of Morris and Thurston[3,4] at East London College, and those carried out at the National Physical Laboratory of Great Britain[5]. The results differ markedly, even as to the essential characteristics. The results obtained at the National Physical Laboratory agree among themselves very well. They are shown on curve I of the appendix. Föppl's results are about 25 percent lower and the shape of his curve (curve II) is entirely different. Morris and Thurston obtained the results shown in curve III. Eiffel[6] gives results for two cylinders only and no correction is made for the ends so a comparison can not be made. Some work was also done at the Massachusetts Institute of Technology[7] but here also no correction was made for the ends. All the investigators mentioned consider the range only from VL = 0 to 5 on the foot second system. (0 - .465 on the metric system). L is taken as the diameter of the cylinder. Thus the need of some more extended work is evident.

The present work was carried out chiefly at higher values of VL, tho there is some overlap. Cylinders of 1, 1 1/2, 2,3,4,4 1/2, 5, 5 1/2, 6 inches (.0254 to .1524 meters) were

used with velocities from 15 to 85 miles per hour. (24.2 - 137 km/hr). The range of values of VL was from 2 to 30 in foot-second units. (.18 - 2.78) in metric units). The cylinders were made of wood with the exception of three brass ones, 1", 1 1/2", 4"; and additional 1" and 4" cylinders of wood were also used. The results are expressed by plotting the coefficient C against VL as a base. L is taken as the diameter of the cylinder. Both metric and English systems are given on the plots. In general the results show that the law in its present form does not represent the facts.

II THEORETICAL CONSIDERATIONS

"Perfect Fluid" Theory

The earliest attempts at theoretical investigation of the flow of fluids followed the usual course of problems in mathematical physics. Just as in our ordinary dynamics the concept of a particle of matter developed, so here the concept of a fluid particle came into being; and just as in our ordinary dynamics friction was for the time being neglected, so here the forces of viscosity were neglected. Thus there arose the idea of a perfect fluid with certain general characteristics which seemed to approximate an actual fluid as closely as ordinary dynamics approximates facts. The properties of this imaginary fluid were then studied mathematically. It was very soon found that the flow of such a fluid

bore absolutely no resemblance to the actual flow, except in a very few special cases. Thus, for example,[8,9] the flow about ~~the~~ a cylinder or sphere, as deduced from this theory, is perfectly symmetrical at front and back; the pressures are symmetrical and there is no resultant force. Thus there is no resistance to motion, a result altogether out of accord with experiment.

Theory of the Surface of Discontinuity

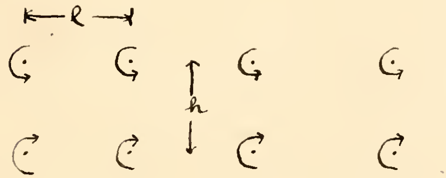
The theory was then modified. It was noticed that in some problems, for instance that of the flow around a flat plate, the mathematical analysis gave negative pressures, a state of affairs physically impossible. It was then postulated that before such a state of flow was reached the fluid "broke" along certain surfaces, these surfaces remaining as surfaces of discontinuity, the air between them being "dead", i.e., at rest, with a constant pressure throughout. Thus, according to this theory, the fluid instead of bending around the sharp corners of a plate with infinite velocity and infinite negative pressure would shoot past and leave a mass of still air behind. This theory proved amenable to analysis[10,11,12] by means of certain processes of mapping on a complex plane and calculations were made for many cases. It was found that there was a resistance offered proportional to the square of the relative velocity of fluid and body, thus far agreeing with experiment. This was a great step in

advance but further experiment soon showed essential differences between theory and fact. For instance, it was found by experiment that the flow around an aerofoil was most sensitive to changes in its upper surface. On the theory of surfaces of discontinuity there is "dead" air at the back, and changes in the upper surface can have no effect. Further, it was found that a region of "dead" air did not actually exist, but on the contrary that there was a region of violent turbulence at the back. Again, the computed values for the resistance absolutely disagreed with experimental values. A little later the mathematicians working on the problem showed that it was impossible for such a surface of discontinuity to be formed in a finite time in a perfect fluid, and that, if formed, it was highly unstable. In fact it was shown that the surface of discontinuity was equivalent to a vortex sheet and tended to "roll up", so to speak, into a series of isolated vortices.

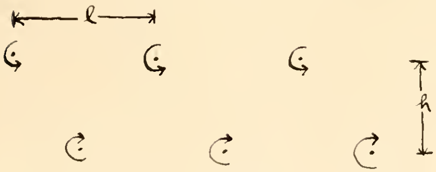
The Work of Kármán

By this time different experimenters had succeeded in taking photographs of air flow past various bodies, and these photographs invariably showed vortex motion at the rear of the body, except in the case of "stream line" bodies. The next mathematical attack on the problem was a flank one. It is impossible so far to solve the general equations of a vis-

cous fluid and thus to compute the flow. By a vortex system similar to that observed was assumed in a perfect fluid and the resultant motion was studied. Kármán[13] was one of the pioneers in this kind of investigation. The first problem discussed was the stability of several vortex arrangements. It would seem that the most probable arrangement in the case of a symmetrical body would be two parallel rows of vortices equally spaced, the vortices in one row being exactly opposite those of the other, and rotating in opposite senses. Kármán showed that such an arrangement was always unstable no matter ~~no matter~~ what the spacing.



Hence if such an arrangement were formed, any slight disturbance would make it pass over into some more stable form. The next arrangement most probable is two parallel rows, staggered with respect to each other by a distance equal to one half the distance between two vortices of the same row.



It was found by Kármán (the calculation is given in full in a paper by de Bothezat shortly to be published in the report of the National Advisory Committee for Aeronautics) that this system is stable, provided the ratio of the distance between

the rows to the distance between two vortices in the same row ($\frac{h}{l}$ in the figure) has a certain value, namely .283. Measurements on photographs of actual flow at a distance from the body gave a mean of .29. Several assumptions made in Kármán's calculations should be noted at once. In the first place the viscous forces are neglected, altho we are not sure that they may not seriously modify the arrangement of the vortices and their spacing. The few experiments made seem to indicate, however, that this omission is warranted, and Levy[14] has also shown mathematically that the effect of the viscous forces on the arrangement is negligible insofar as the motion at a particular instant is concerned. In the second place, the most serious objection is that the vortices are assumed to be of infinitely small cross section. This is undoubtedly not true in practice; and the fact of finite section probably accounts for many phenomena observed by the author.

Kármán, after discussing the stability of these systems, then proceeds to compute the resistance of a body forming such vortices, by computing the momentum lost in giving off two vortices. He finds that the resistance is given by the formula which has already been experimentally verified for a large number of cases, namely $R = C\rho SV^2$, S being the area, ρ the density, V the velocity, and C a coefficient, non-dimensional, dependent on the configuration of the vortices, hence upon the shape of the body. His calculated values agree closely with the experimental values of Föppl[2]; but,

unfortunately, Föppl's values are not in agreement with more recent values, tho of the same order of magnitude.

It is to be noted that the measurements made by Kármán are on photographs of the flow of water, whereas the results of Föppl are on wires in air. Furthermore the values of $\frac{VL}{\nu}$ in the two sets of experiments are widely different so that in any case the results are not comparable. The author finds for a value of $\frac{VL}{\nu}$ equal to that in Kármán's experiments, a value of the coefficient in good agreement with Kármán's value. Thus it is evident that Kármán's picture of the flow is close to the true state of affairs in some cases.

Consideration of Fluid Stresses

The noticeable thing about the preceding investigation is that it is a flank attack. We would like to know the mechanism of formation of these vortices and a great many other things about them. The physical properties which surely determine these things, physical properties which have been entirely overlooked, are the stress characteristics of the fluid. It was this omission that caused the potential theory to fail; and, before we can make a direct attack on the problem, we must study something more about these fluid stresses. We are familiar with the fact that in an elastic solid, when the stress reaches a certain value, conditions change entirely. When the elastic limit is reached the phenomena are essentially different. So in a fluid when the stress reaches a

certain value, it will "break" and behave quite differently. Dr. de Bothézat[15] has worked up this aspect of fluid dynamics perhaps more than anyone else and his conclusions are of great interest.

In an elastic solid the factor determining the stress is the strain, i.e. the displacements of points in the neighborhood of some point relative to that point. In a fluid on the other hand, the determining factor is the time rate of change of this quantity; that is, the velocity gradient, depending upon the velocities of points in the neighborhood of a point relative to that point. At every point of the fluid there is a certain stress determined by this; and, when the stress reaches a certain value, the fluid will break up into separate parts (as in the crest of a wave where the stress owing to the weight breaks the wave). This condition is unstable and passes over into a vortex system, probably thru the intermediate stage of the surface of discontinuity.

A specific illustration may make this clearer. In the case of a cylinder it is not inconceivable that at very low speeds, streamline flow results, and that the drag force is only that owing to skin friction. As the speed increases, the stress increases, until finally the fluid breaks up into separate particles; immediately the pressure behind the cylinder rises as the fluid there comes to rest. The stress is relieved and a surface of discontinuity is formed. This breaks up into the vortex system described before. The question at once arises

in the case of a cylinder, since the streamline flow is symmetrical, as to why the fluid breaks at the back and not at the front. This is evidently owing to the viscosity. The effect of viscosity will be to slow up the fluid and thus increase the strain at the back. Hence the stresses at the back will be greater than those at the front and the fluid will break at the back first.

It is of course evident why the vortex system is of the "staggered" type. Kármán showed that the "symmetrical" type was unstable, and the physical reason for this instability is that any slight disturbance makes the flow unsymmetrical. After the disturbance is over the flow changes back to its original form but owing to the inertia becomes unsymmetrical in the opposite manner. Thus a periodic change in the flow is set up, which prevents the simultaneous formation of the vortices. The vortices are formed alternately on each side.

A word might be said about the phenomena at the surface of the body. At the surface itself the velocity of the air must be zero, but at a short distance it may have a high value. Hence in this layer there is a vortex sheet, and the same processes of breaking taking place. The energy dissipated here accounts for the skin friction. It is probable that this same process repeats itself at a higher speed. This new flow involving vortical motion also produces stresses, and in time these will again rise to a critical value. The existence of a second critical velocity has actually been observed.[16]

Dimensional Theory

The subject of forces as distinct from the actual flow has been considered from a slightly different standpoint, namely from the standpoint of dimensional theory. According to the usual derivation[17] it is assumed that the force can depend only upon the velocity of the fluid, its density, its viscosity, and upon the dimensions of the body. Thus on writing down the dimensional equation, we have that

$$\left(\frac{L}{T}\right)^{\alpha} \left(\frac{M}{L^3}\right)^{\beta} \left(\frac{M}{LT}\right)^{\gamma} L^{\delta}$$

must have the dimensions of a force $\frac{ML}{T^2}$. Hence we have as conditions to determine $\alpha, \beta, \gamma, \delta$

$$\beta + \gamma = 1$$
$$\alpha - 3\beta - \gamma + \delta = 1$$
$$\alpha + \gamma = 2$$

These are not sufficient to determine all four quantities but expressing the three others in terms of γ we have

$$\beta = 1 - \gamma$$
$$\alpha = 2 - \gamma$$
$$\delta = 2 - \gamma$$

Hence writing ρ for density, V for velocity, L for a linear dimension of the body, and K for the coefficient of viscosity, the force is of the form

$$\rho L^2 V^2 \left(\frac{K}{VL\rho}\right)^{\gamma}$$

or, since y is indeterminate,

$$\rho L^2 V^2 f\left(\frac{\nu}{VL}\right)$$

when ν is the kinematic viscosity coefficient, equal to $\frac{\kappa}{\rho}$, and f is some function as yet undetermined. Hence, if this theory is correct, the coefficient of resistance should be a function of $\frac{VL}{\nu}$ only, or if air is used, of the product VL only, independent of V or L separately. The theory has apparently been verified in some cases, but this is no guarantee for its truth in all cases.

Several criticisms, very vital ones, may be at once offered. First, the results are wrong if any factor has been overlooked which affects the force. Experiment is the only way of deciding such a point. In the second place it seems to the author that a mistake has been made in applying the theory itself. We are studying phenomena going on in the fluid; and it seems perfectly logical that we must then confine ourselves to properties of the fluid itself. What *a priori* reason have we to believe that the length of the body can *directly* affect the force, any more than the density of the material of which the body is made? The length of the body can only affect the force if it changes some length in the fluid, for instance the distances between the vortices in the distribution mentioned before. Thus we must remember that the L in the formula is a property of the fluid, not of the body and we can substitute one for the other only in case one is always the same

function of the other. That this substitution is not always permissible appears from the experiments to be described. The flow is not always similar in the case of different dimensions of the body, as the dimensional theory assumes, without definitely saying so.

The remarks made previously about stresses seem to indicate something antagonistic to this form of dimensional theory. For, in order for stresses to be the same, $\frac{V}{L}$ must be constant, (L being again a length in the fluid). Thus it appears that, if there are critical velocities, i.e. velocities of flow at which the nature of the flow changes, they should come at constant values of $\frac{V}{L}$ if the theory of critical velocities is correct. We must remember, however, that in such cases of critical velocities the stress may be constant over a large area and the break may take place simultaneously over this area. In this case the force and critical value will depend not only on the stress but also on the area over which the break takes place, hence, on the whole, on $\frac{V}{L} \times L^2 = VL$. Thus, unless the break takes place only at a single point, stress considerations yield the same law of similarity. We must however remember that the dimensional law fails if we have overlooked any factor.

III FORCE MEASUREMENTS

Apparatus

The Tunnel

The entire wind tunnel facilities of the Bureau of Stan-

dards were placed at my disposal by Dr. Briggs. As there is no published description of the tunnel it may not be out of place to give a brief account of its principal features here. The tunnel, similar to those at the National Physical Laboratory, is contained in a large room, the air being drawn thru the tunnel by a four-bladed 9 foot propeller and returning thru the room. The room is 69 ft. 10 in. long, 18 ft. high and 30 ft. 4 in. wide. The tunnel itself has its axis along the long axis of the room, is 45 ft. 6 in. total length, the propeller tips being 13 ft. from one end of the room. The working part of the tunnel is straight, octagonal in section, 53 3/4 inches between opposite faces, and is 25 ft. 4 1/2 in. long. This portion is built of wood supported by a metal framework. The entrance consists of a wooden framework 4 ft. long covered with airplane cloth, rounded off to admit of easy inflow. The exit end consists of a cone 15 ft. 1 1/2 in. long, 9 ft. 1 3/4 in. in diameter at the outer end, i.e. approximately 9° half angle after allowing for a small straight part at each end where the junction is made. This exit cone is built up of a wooden framework covered with airplane cloth. A wooden diffuser is used around the exit end. Two honeycombs are used to straighten the air flow, one at the exit end of the working portion, the other 2 1/2 feet from the entrance end of the working portion. The propeller is driven by a 100 H.P. D.C. motor. The motor is controlled both by inserting resistance in the armature and by inserting resistance in the field, and may be run with the arma-

ture either on 110 or 220 volts. The line voltage is only fairly constant, on some days fluctuating considerably, on others being very steady. A traverse of the tunnel showed variations in velocity of as much as 2%, altho for the most part they were less than this, a fair average for the part occupied by the models being .6%. This traverse was taken at one section only; for due to the stress of war work no others have been made.

The Balances

The tunnel is equipped with two balances, one for aerofoil work, the other for heavier work. The first balance is similar to the National Physical Laboratory Balance[18] in every way except as to the means of taking torque measurements. It is sensitive to .0001 lb. The largest forces which can be measured on it are 5 lbs. Hence it was impossible to use this balance for all the cylinders; and, as it was desired to obtain results whose relative values were accurate, measurements on this balance were discarded in plotting my curves. The other balance consists of a system suspended by two thin steel strips, and measures the force along the wind primarily. If lift measurements are desired, moments can be taken about a second set of blades, and from these the lift force may be computed. In this work the second set of blades was not used. The balance is sensitive to .001 lb., and will take forces up to the strength of the blades. All of the points plotted were obtained on this balance. It must be remem-

bered that both balances measure moments only, and actual forces are obtained by assuming the force to act at the center of symmetry of the body. Only bodies with a horizontal plane of symmetry can be used.

Speed Measurement

The speed was measured by means of the usual static plate and inclined gauge. This particular gauge was constructed with unusual care. The glass tube was a pyrex tube straight to .002 inch. The ledge on which it rested was straight when put on to ± .002 inch and the tube was held down on the ledge to within ± .005 inch. The liquid used was benzene, as this obviates trouble from dirt and grease which is always present with water. Since benzene attacks rubber it was necessary to use special connectors, and, since it has a high coefficient of expansion, it is necessary to observe the temperature and to make a correction. This gauge was calibrated against a U tube of large diameter containing benzene and read with a cathetometer. The static plate was calibrated against a standard pitot tube placed at the center of the tunnel. The calibration was a remarkably good one and it is certain that the static plate gives the velocity at that particular place to a few tenths of one percent. The gauge was very steady except at very high speeds, the oscillations as a rule being slow enough to read the gauge well within one percent. At very low speeds the accuracy is of course not so

great.

It must be remembered that a Pitot tube gives us the quantity $1/2\rho V^2$ only, where ρ is the density of the air. It is not usual in aerodynamic work to make the calculation for V every time. Since the forces are assumed to vary directly as ρV^2 this quantity alone is computed. If a value for V is given, it is determined from this, using "standard" density. Thus the true value of V is not obtained. (The "standard" density used at the Bureau of Standards and also at the National Physical Laboratory is .1223 gms/cm^3 at 15.6°C, 760 mm. pressure.) This is unfortunate when we consider the general formula for the force = $\rho s V^2 f(\frac{VL}{\nu})$. For a change of temperature does several things whose effect may be shown by an example. Suppose we take measurements at 25°C instead of our standard 15°C. Making the Pitot reading identical in the two series of measurements, $1/2\rho V^2$ is the same for both temperatures. On the other hand ρ has changed. V has actually been increased by about 2% (density being less by about 4%). ν, equal to the $\frac{\text{viscosity (static)}}{\text{density}}$ has also changed. The static viscosity has increased by about 3 1/2%, the density decreased by 4%, hence the kinematic viscosity as a whole has increased by 7 1/2% approximately. Hence $\frac{VL}{\nu}$ has been decreased, or V L (if we have assumed ν constant) has been decreased by 5 1/2%. It has not been considered necessary to make this reduction since no investigations have been made on the change in aerodynamic forces with temperature, and since the dimensional law

itself in present form is not true. Likewise no records of pressure or of moisture content were made, and these affect the density also. Other investigators do not state whether they make such correction or not. It seems important only in the exact location of critical speeds, where the coefficient of resistance changes rapidly with VL.

The Models

The models have already been described in a general way. The wooden ones (1". 2", 3", 4", 4 1/2", 5", 5½", 6") were turned by E.E. Leach, a pattern maker of Baltimore, Md. They were accurate to .01 inch, both as to being circular and as to being straight. The wood was white pine, and the surfaces were coated with shellac. The brass ones were made of commercial brass tubing and were accurate to .005 inch. All were approximately 18 inches long. The dimensions are given in table I of the Appendix. They were held on the balance arm by means of a 5/16 inch steel spindle.

Methods of Measurement

It was advisable from the theoretical standpoint to obtain results applicable to infinite cylinders. To secure this result, the "guard ring" principle was used. Two short cylinders were placed in line with the cylinder on the balance, one being suspended from the roof of the tunnel and the other being on the spindle. The top guard was in line with the cyl-

inder when the balance was in its zero position and cleared by just enough to allow the necessary play. These guards were 3 inches long. Tho this is rather arbitrary, it was found experimentally that this length is quite sufficient. Measurements were taken as follows. (Two observers required.) With the cylinder and one guard on the balance arm as described, the observer of velocity signaled when the velocity had some definite value. The observer at the balance adjusted the weights so that the beam was on the average in the zero position. This was repeated for the whole series of wind velocities. Then a second set of readings was taken with only the lower guard on the balance, the cylinder being suspended from the roof over the guard so as to secure the same flow. The second set of readings was taken at approximately the same speed as the first. In computing, the second set was reduced to exactly the same speed as the first, assuming the square law over this short range. Due allowance was made for the length of the balance arm, as explained in the section on balances. No windshield was used, as it was thought best to avoid any possible interference caused by it.

Results

The results of primary interest are plotted on Curve 4. Here are shown measurements on the 1" brass cylinder, the 1 1/2" brass cylinder, the 2", 3", 4", 4½", 5", 5½", 6" wooden cylinders and the 4" brass cylinder, all taken on the same balance,

so that the results are comparable. The first noticeable thing is that for cylinders of diameter below 2" the resistance coefficient depends not only on the product VL but also on L. The force on a 1" cylinder is half again as large as that on a 2" cylinder for the same value of VL, thus indicating an essential failure of the present dimensional law. On the other hand, it is noted that the coefficient for the 1 1/2" cylinder, while 10% greater than that for the 2" cylinder, for values of VL up to 8 Ft^2/Sec., coincides with that for the 2" cylinder for values of VL beyond 12 Ft^2/Sec., denoting that some critical change takes place in the flow about the 1 1/2" cylinder at this point. Finally, it is to be noted that for values of VL in excess of 28 Ft^2/Sec. all of the curves show a drop. Indications are that a critical value of VL for all the cylinders is being approached. These are the essential features; and it might be pointed out that none of these are entirely new. For, looking at the figures given by Norris and Thurston (see Curve 3) it is noticeable that the 1 1/4" cylinder gives a coefficient much less than the 1" and the 1 1/2" less than the 1 1/4", and so on to 2". Their values and the author's are as follows:

L	VL Ft^2/Sec.	Norris and Thurston	Author
1"	2.39	.56	.61
1 1/2"	3.67	.50	.55
2	4.91	.44	.40

The agreement between the relative values is striking; the absolute values differ by 10%. Furthermore, altho the British investigators do not carry their measurements to diameters greater than 1 1/4", a careful inspection of their figures shows that the coefficient for the 3/4" cylinder is slightly higher than that for the 1 1/4" cylinder even for values of VL approaching to the first critical value at VL = .25 Ft^2/Sec. Finally altho the work at the Massachusetts Institute of Technology can not be directly compared, the results obtained also show that the coefficients for a 3/4" cylinder are definitely higher than those for a 1" cylinder and that those for the 1/2" cylinder are definitely higher than those for the 3/4" cylinder. Thus the dependence of the resistance coefficient on size at identical values of VL has been shown before, tho no one has definitely pointed it out.

Secondly the existence of the second critical velocity has been noted by G. I. Taylor in a Confidential Report[19] issued by the British Advisory Committee. Taylor was measuring the pressure distribution on a 6" cylinder and found that approximately at a velocity of 40 Ft/Sec. (VL = 20 Ft^2/Sec) the characteristics of the pressure distribution changed. His results were qualitative only and it is seen from the present results that the change occurs at VL = 26 Ft^2/Sec. As far as is known, the peculiar behavior of the 1 1/2" cylinder has not before been noted.

Certain other results might be noted. The [illegible]" cylinder was freshly coated with a wax mixture and a test was made immediately afterward. The resistance dropped by approximately 3%. After standing for a week, a second run was made and the resistance was found to have its original value. 3% is not much beyond the errors of experiment, and it would therefore seem that waxing the surface has little effect. The wood and brass cylinders check within the experimental error, so that Zahm's conclusion[20] as to the independence of skin friction on the surface so long as the surface is not visibly rough seems justified. In investigating the accuracy of the results, some measurements of the "end effect" were made. On the 1" cylinder it was found that the omission of the guards decreased the force by approximately 10%. On the 4" cylinder, on the other hand, the omission of the guards made practically no difference, it being less than 1%.

This was tried with the cylinder both vertical and horizontal. It was thought at first that since the balance measures moments only, that the change in the force might be large altho the change in the moment is small. The test with the cylinder horizontal shows that this is not the case, for with the cylinder horizontal we know that the force acts in the same horizontal plane whether the guard is present or not. The values of the coefficient derived from this cylinder with no guards check very well the values with the guard. Now the British investi-

tors found[21] for very small wires that correction could be made for the end by using in the calculations a value of the length of the wire four diameters shorter than the actual length. Thus it appears that the end effect does not increase for the same length proportional to the diameter but that certain peculiar changes take place in the flow about the ends.

Lastly, on looking at the points for the 1 1/2" cylinder it is seen that where the curve is dropping, the points fall into two sets. These two sets were taken on different days, the temperature on the two days being different. The stresses in the fluid undoubtedly depend upon the temperature, and it is not to be considered remarkable that under such conditions the change in flow should occur at different points. This whole question of the effect of temperature changes on air flow, especially as to the effect on the forces and as to the effect on the critical points, is one that deserves further study. For instance, it may be possible that the stalling angle of an airplane is different on a hot day from that on a cold day.

Accuracy

Altho some investigators in wind tunnel experiments claim an accuracy of as much as 2%, it is extremely doubtful whether a greater accuracy than 5% can be obtained in relative values, and it is highly improbable that an accuracy of 10% can be obtained in absolute values with present methods. It is for this

reason that most stress has been laid on relative accuracy, relative characteristics, and relative values. Let us first consider some of the things which limit the relative accuracy. In the first place, there is the question of the very nature of the quantity to be measured. The effect of the vortex flow, which has already been described, is to produce a force whose magnitude is continually changing. Under ideal conditions, with vortices formed at a uniform rate, the force would be periodic and it ought not to be hard to detect the periodicity at slow speeds. But great complications are introduced by the fluctuations of speed in the tunnel. These entirely disrupt the periodicity and cause what may best be described as an irregularly varying flow. Thus what we attempt to measure is a time average of the force. Now in such cases the accuracy of the measuring apparatus is of no advantage beyond a certain point. In fact, too great sensitiveness may be undesirable. To make the measurement more troublesome, the amplitude of this irregular variation is very sensible compared to the absolute value of the average force. The conditions of measurement are entirely similar to those prevailing at the "burble" point in aerofoil measurements. Hence even relative accuracy is not large, and the only basis for claiming it is that the measurements repeat and fit a smooth curve that well.

In the second place, the question of guards offers difficulty. It has already been mentioned that the length was

found entirely sufficient. The only other question is as to their alignment. It is of course physically impossible to align the guards exactly and the flexure of the cylinder with increase of speed soon changes any accurate alignment which may have been made with the air at rest. Hence since the 1" cylinder had already shown the greatest end effect, the effect of lack of alignment of the guards was tried with it. It was found that moving the guard towards the direction from which the wind was blowing decreased the force, moving it in the opposite direction increased the force, the total change on moving the guard from a position 1/4" toward the front to a position 1/4" toward the back being 3 or 4% of the total force. Thus in the actual experiment the error owing to lack of alignment cannot be greater than 1%, since a shift of 1/8" would be unusual.

In the third place, there is the error owing to the flexure of the cylinder. Owing to flexure the lever arm of the weight of the body will be changed and the force will be apparently too high. The error owing to this cause is slight, since the weight of the cylinders is not great and the flexure is small. Finally, there are the usual avoidable errors, of mistakes in reading or computing.

As to the absolute accuracy, it may seem strange that no greater accuracy than 10% is claimed, but here again a certain possibility of error enters in, which is very difficult to eliminate. This is the uncertainty as to the distribution of vel-

ocity in the tunnel and as to the effect of such distribution on the force. The Pitot calibration gives the velocity at some one point. Now it is possible for the distribution of velocity to be of such a nature that great errors may be introduced. For we measure moments, only, and if the irregularities are such that the velocity is low on both sides or high on both sides, as is frequently the case, our resultant moment will be incorrect.

The error due to this cause might amount to as much as 4% in an extreme case. But in addition to this primary effect of variation in velocity across the section there is the secondary effect on the flow and the consequent effect on the force. Of this we know nothing but it is possible that the flow may be so modified in such a manner as to introduce large errors in the force. Measurements made on the first balance referred to in the section on balances show a uniform difference of approximately 10% when compared with those made on the second balance under similar conditions; and it is for this reason that no greater accuracy is claimed. This question of velocity distribution is undoubtedly responsible for the lack of agreement among some of the investigators mentioned before, and can not be allowed for. Even if a very accurate traverse of the tunnel is made, one is sure that the model has changed the distribution. This discrepancy did not occur in the case of some bombs tested here but there were only 4 or 5 inches in diameter and did not take up much of the tunnel. In conclusion, the relative accuracy

of the present results is well within 5%, and altho the absolute values check the British well within this figure, they are not certain to more than 10%.

IV Pressure Distribution Measurements

Method

Owing to the peculiar changes in the force characteristics of the cylinders, it was felt advisable to undertake the measurement of the pressure distribution over the cylinders and see if this would not throw some light on the changes taking place. The measurements are only rough, but they yield some interesting results. The method was a very simple one. A single small hole was drilled into the cylinder at a distance of about six inches from one end. This hole could be placed in any position relative to the wind stream by simple rotation of the cylinder. A special fitting was made, consisting of an iron pipe with the cylinder spindle screwed in the top and a bearing in which the pipe could turn. A pointer was fixed to the bearing and a divided head to the pipe so that the angular setting could be read off. The hole was connected by means of a rubber tube passing thru the top of the tunnel to one side of a slant gauge, (the same as was used for speed measurements in the work on forces). The other side of this gauge was connected to the static opening of a Pitot tube placed in the tunnel a little below the level of the center of the tunnel so as not to interfere with the flow about the hole. Thus read-

ings were obtained giving the difference between the actual pressure at any point on the cylinder and the pressure that would prevail if there were no cylinder present. The speed can of course be determined from the maximum pressure difference on the front of the cylinder, which is $1/2\rho V^2$, but readings were taken by means of the static plate connected to a second inclined manometer. This second manometer has a comparatively large slope and contained water so that its indications are not very accurate. However it served to enable the observer to keep the speed fairly constant.

It is to be noted that this method of making a hole in the surface does not necessarily give us the true pressure on the cylinder. We have no idea as to the modifications the hole may introduce. We know that there is on the surface of the body a vortex layer, which causes the phenomenon of skin friction. Now vortex layers may sustain a very great difference in pressure, as for instance in the case of the vortex layer bounding the slip stream of a propeller. Whether the hole breaks thru this vortex layer and gives us the pressure outside it, or whether it gives some other pressure we do not know. However, this method will give us some indication as to whether the flow changes or not, and that was its present purpose.

The diameter of the hole was in all cases 1/16 inch. Thus we get simply an average over a certain section. Smaller holes were not used because of the great friction introduced

making readings uncertain and taking more time. The cylinders used were the 1", 3", 4 1/2", and 6" wood cylinders; of these the 1" was run at four speeds, the 3" at two speeds and the others at one speed. In all cases evidence of the fluctuations of the flow was evident in the fluctuation of the gauge.

Results.

Since the results are not very accurate the readings were not reduced to absolute pressures, but the gauge reading itself was used, it being proportional to the pressure. It is usual in plotting such results to plot from a circle as base, laying off the pressures at the various points along the radii thru these points, negative differences being toward the center. In this paper, to enable a larger scale to be used, the negative pressures are plotted outwards the same way as the positive ones, but no confusion need arise if it is remembered that on the front (toward the wind) the pressure is greater than the static pressure, while on the back (and also part way on the front) it is less than the static pressure. These curves are shown in the Appendix, Nos. 5, 6, 7, 8, 9, 10, 11, 12. The ordinates are gauge readings. To get absolute units it is only necessary to take the pressure on the front as .500 and the others in proportion.

At first sight no essential difference appears. In all the pressure becomes equal to the static pressure at an an-

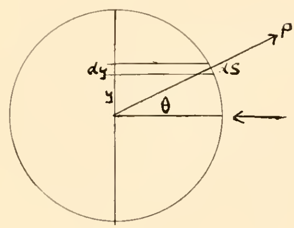
gle of about 40° from the wind direction. On all the pressure drops below static very quickly afterward, the maximum having a characteristic form and occurring at nearly the same angle, 65°-70°. On all the pressure drop on the back is nearly uniform. So that apparently there is no violent difference in the flow. Yet if we examine the curves more closely, as inaccurate and irregular as are some of the points, one fact becomes evident. This is that the relative size of the hump on the front and the hump on the back, in other words the ratio of the pressure increase on the front to the pressure drop on the back, is very different for the 1" cylinder from that for the others. The figures are given in Table XIII with other things, but they are repeated here.

Diam. of Cyl.	Ratio of maximum increase in pressure on front to average decrease in pressure on back.
1"	1.10 1.19 1.20 1.05 (at different speeds)
3"	1.74 1.64
4 1/2"	1.47
6"	1.54

Tho the variation for any one cylinder is great, (15%), yet the difference between the 1" and the others is much greater, (30% or more). Furthermore, the three others are within 20% of each other. Thus there can be little doubt that the high

value of the resistance for the small cylinders arises from some difference in the flow at the rear of the cylinder which causes the pressure on the rear to be further reduced below the static pressure.

It was decided to integrate the pressure over the surface and see how well this checked the actual force. For this purpose the second series of curves were plotted. Suppose at any point of the cylinder our pressure is P. Its



contribution to the component of the force along the wind is $P ds \cos\theta$ (see figure), but $ds \cos\theta = dy$. Hence the total force is $\int P\, dy$. Thus, if we plot P against y, or what is the same thing against $\sin\theta$, or what is again the same thing, against the cosine of the angle of the surface element to the wind, the area of our curve will represent the total force in the direction of the wind. These areas were measured with a planimeter, the results being given in Table XIII. From these areas, knowing the scales, the force on the cylinder could be computed. The results of such computations are shown in the same table. Little can be inferred as to the results, because of the uncertainty as to the velocity.

Accuracy

It is apparent that the pressure measurements do not

have a high degree of accuracy. Several things prevent accuracy. In the first place, the pressure measurements were made at a point in the tunnel between the two balances, and all the uncertainty as to the velocity distribution across the section enters. In the second place the pressure behaves as does the force, varies irregularly, especially at the point of maximum pressure decrease and on the back. This is evident from the plots. If we neglect the maximum pressure value of the velocity as being more unreliable than the value obtained from the readings of the static plate, we find that the calculated coefficients do not differ much from the ones observed on the balance used in the force measurements. On the other hand, if we use the maximum pressure values, we get fair agreement with measurements on the second balance. Hence we can place little dependence on the pressure integration and must regard the curves simply as giving us a general idea of the distribution. The prominent features are the general shape of the curves, the difference in this shape in the case of large and small cylinders, and the very great unsteadiness of the six inch as showing the approach to the critical velocity.

Reference has already been made to the work of Taylor[19]. He found at higher speeds that the pressure decrease reached a large maximum at 90° away from the wind, and that the constant pressure prevailed from 120° to 180° away from the wind. Fage[22] also made some pressure measurements on a 2" cylinder and found

that the length of the cylinder modified his results profoundly. He tried a cylinder with boards approximating an infinite cylinder, and also one eighteen inches long. The principal difference was that in the case of the infinite cylinder the ratio of the maximum pressure on the front to the average decrease on the back was about 1, whereas in the case of the 18" cylinder it was 2 1/4. Now in the present work the value is nearer 1 than 2 1/4, so that it seems there is quite a difference between the tunnels.

V CONCLUSION

It is thus seen that the flow about a 1" cylinder, 1 1/2" cylinder, or 2" cylinder is different in some respects from the flow about a cylinder of higher diameter. This makes itself evident in the forces by the fact that the resistance coefficient is a function of the size as well as of the parameter VL. It shows itself in the pressure measurements by the fact that in the case of the smaller cylinders the decrease in pressure on the back is greater in proportion to the increase in pressure on the front than on the large cylinders. Notwithstanding this the flow must be of somewhat the same nature since the form of the pressure distribution curve is not materially changed. The next question is as to the best hypothesis to explain these facts. Whatever the factor that has been overlooked in the past, it must satisfy these conditions and furthermore must become negligible or constant for both low and high values

of the diameter. For it has been seen that the difference between forces for all diameters below 1" is small and that for all over 3" is negligible.

There seems to be one thing that may explain these facts tho there is no absolute proof. The factor that may cause these changes is the finite size of the vortices which are formed behind the cylinders. For, when two circular vortices come close enough together, the separate parts of each vortex will have different velocities, the vortex will therefore be distorted, and we can no longer treat it as a filament. Thus if our body is made smaller and smaller, the vortices will be brought close enough together for this action to take place, and the character of the flow will be altered. The whole question might be settled by taking photographs of air flow past different size cylinders, or by making the necessary mathematical calculations.

This hypothesis seems to satisfy most of the requirements. It will presumably give a flow which is only slightly different from one where only filaments are present. When the body is large, the distortion disappears as the vortices are not close enough together. As we make the body smaller, the interaction becomes greater and greater. As it requires energy to distort the vortices, it would seem that the effect would be to increase the force. When we get to a certain point the distortion reaches a certain maximum, and finally the distortion is so great that the vortex system can not exist. Thus such a hypothesis would account for the general

characteristics, two there may be some other one which would do the same. This is a matter for further investigation.

The peculiar behavior of the 1 1/2" cylinder, and the occurence of the second critical velocity still remain to be explained. Not enough work has been done to justify any definite hypothesis; but it is possible that there may be several distorted forms and that as the speed is increased the stress becomes so great that one form passes over into another. This, however, is only a possibility. The second critical speed probably occurs when the stresses again reach their critical value and the fluid breaks. What happens then is beyond our knowledge until we have a tunnel which will enable us to get higher values of VL. Six inches is already very large for a 4 1/2 ft. tunnel, and it is useless to go higher by increasing the size of our models. Furthermore, because of the unsteadiness of the velocity of the wind, measurements on a 6" model could not be taken above 50 miles per hour.

In conclusion I desire to express my indebtedness to Dr. L. J. Briggs, who provided every facility possible and was always interested in this investigation, my thanks to Dr. J. S. Ames (the inspiration to do the work came from him) for his friendly criticism, and my obligations to Dr. de Bothézat who talked over the subject matter frequently with me. To Mr. Alfred McCurdie and Mr. Gregory Breit who assisted in the ob-

servations are due much credit for the care with which they did their work. It is hoped that other men will take this matter up and learn more about it. During the next year further work will be done at the Bureau of Standards.

Johns Hopkins University.

June 1, 1919.

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Biographical

Hugh Latimer Dryden, son of Samuel Isaac Dryden and Nova Hill (Culver) Dryden, was born in Pocomoke City, Maryland, on July 2, 1898. He attended the public schools of Somerset County until 1907. His elementary education was completed in the public schools of Baltimore. In 1911 he entered the Baltimore City College, graduating in 1913. He matriculated at the Johns Hopkins University in the fall of the same year. The degree of Bachelor of Arts was awarded to him in 1916. During the years 1916-1917 and 1917-1918 he pursued graduate work in Physics, Mathematics, and Geological Physics at the Johns Hopkins University. He attended lectures in Physics under Professor Ames and Professor Pfund and lectures in Mathematics under Professor Morley, Professor Cohen and Professor Coble. He engaged in some special reading under Professor Reid. During the years 1916-1918 he was laboratory assistant in Physics, and during 1917-1918 lecture assistant to Professor Ames. The degree of Master of Arts was conferred on him in 1918. He was awarded a fellowship for 1918-1919 but resigned to enter upon war work at the Bureau of Standards. He was granted leave of absence and continued his studies under Professor Ames. The work submitted for a dissertation was done at the Bureau of Standards.

A P P E N D I X

Tables and Curves.

TABLE I

Dimensions of Cylinders

Nominal Diameter Inches	Material	Average Diameter Inches	Average Diameter Meters	Average Deviation from Mean Inches	Average Deviation from Mean Meters
1	wood	.9804	.02490	.004	.00010
1	brass	.9985	.02536	.002	.00005
1 1/2	brass	1.503	.03818	.005	.00013
2	wood	1.989	.05052	.004	.00010
3	wood	2.993	.07602	.002	.00005
4	wood	3.990	.10135	.005	.00013
4	brass	4.002	.10165	.005	.00013
4 1/2	wood	4.486	.11394	.004	.00010
5	wood	4.995	.12687	.007	.00018
5 1/2	wood	5.484	.13929	.005	.00013
6	wood	5.991	.15217	.007	.00018

No deviation is greater than .015 in., few greater than .010 inches. Each reading is the mean of 20 readings, two sets of 10 at ends of perpendicular diameters, except in the case of the last two cylinders.

TABLE I, Continued

Dimensions of Cylinders

Nominal Diameter Inches	Material	Length Inches	Length Meters
1	wood	17.97	.45644
1	brass	16.875	.42863
1 1/2	brass	18.11	.45999
2	wood	17.94	.45568
3	wood	17.91	.45491
4	wood	17.97	.45644
4	brass	18.06	.45872 (not round)
4 1/2	wood	17.97	.45644
5	wood	18.00	.45720 (only measured in plane normal to wind; about 1% out of round)
5 1/2	wood	17.97	.45644 (normal to wind)
6	wood	17.97	.45644 (normal to wind)

TABLE II

Forces on 1 in. Brass Cylinder

First Run - April 25, Temp. 24.5°C

VL		C
Ft^2/Sec	$Meter^2/Sec$	
1.94	.180	.584
2.43	.226	.619
3.04	.283	.616
3.66	.340	.627
4.26	.396	.616
4.86	.451	.623
5.48	.509	.620
6.10	.566	.615
6.68	.620	.624
7.32	.680	.611
7.94	.738	.618
8.53	.792	.617
9.10	.845	.621

In this and the following tables, L is taken as the diameter of the cylinder. C denotes the absolute coefficient.

TABLE II - Continued

Second Run - May 12, Temp. 23°C

VL		C
Ft^2/Sec	$Meter^2/Sec$	
1.94	.180	.589
2.42	.225	.589
3.05	.283	.601
3.62	.336	.613
4.28	.398	.607
4.93	.458	.609
5.46	.508	.603
6.14	.570	.608
6.65	.618	.612
7.36	.684	.595
7.91	.735	.601
8.56	.795	.595
9.18	.852	.595
9.70	.900	.597
10.35	.961	.603

TABLE III

Forces on 1 1/2 in. Brass Cylinder

First Run - May 7, Temp. 26°C

VL		C
Ft^2/Sec	Meter^2/Sec	
2.88	.268	.546
3.23	.300	.556
3.65	.339	.537
4.19	.389	.551
4.57	.425	.546
5.05	.469	.554
5.57	.518	.546
5.76	.535	.549
6.48	.602	.541
6.91	.642	.536
7.39	.686	.543
7.84	.728	.539
8.29	.770	.542
8.73	.811	.542
9.16	.851	.535
10.11	.939	.529
11.21	1.041	.520
12.29	1.141	.506
12.95	1.201	.510

TABLE III, Continued

Second Run - May 12, Temp. 19°C

VL		C
Ft^2/Sec	$Meter^2/Sec$	
2.91	.270	.542
3.67	.341	.544
5.62	.522	.545
6.53	.606	.540
7.40	.687	.532
8.30	.771	.525
9.28	.862	.509
10.12	.939	.499
10.96	1.019	.499
11.78	1.094	.490
13.77	1.280	.485
14.56	1.353	.484
15.55	1.444	.476
11.81	1.098	.493
10.10	.937	.499

TABLE IV

Forces on 2" Wood Cylinder

First Run - April 29, Temp. 26°C

VL		C
Ft²/Sec	Meter²/Sec	
3.90	.362	.472
4.90	.455	.482
6.18	.574	.493
7.61	.707	.489
8.48	.788	.494
9.74	.905	.485
10.82	1.005	.483
12.15	1.128	.483
13.42	1.247	.478
15.08	1.400	.464
16.49	1.531	.477
17.27	1.604	.474
18.36	1.706	.471
19.38	1.800	.469

TABLE IV, Continued

Second Run - May 5, Temp. 30°C

VL		C
Ft^2/Sec	$Meter^2$/Sec	
3.88	.360	.503
4.32	.401	.500
4.87	.452	.486
5.68	.528	.484
6.22	.578	.494
6.86	.638	.494
7.66	.712	.493
8.53	.792	.488
9.30	.864	.490
10.01	.930	.491
10.52	.977	.490
11.27	1.047	.486
12.08	1.122	.489
13.33	1.238	.487

TABLE IV, Continued

Third Run - May 12, Temp. 22°C

VL		C
Ft^2/Sec	$Meter^2/Sec$	
3.87	.359	.487
4.88	.453	.493
6.12	.568	.493
7.46	.693	.486
8.58	.797	.490
9.78	.908	.498
10.85	1.008	.496
12.15	1.129	.484
13.39	1.244	.490
14.43	1.340	.486
15.57	1.446	.479
17.11	1.590	.485
18.26	1.697	.485
19.50	1.810	.474
15.62	1.450	.481

TABLE V

Forces on 3" Wood Cylinder

April 14, Temp. 17°C

VL		C
Ft^2/Sec	$Meter^2/Sec$	
5.38	.500	.438
6.12	.568	.409
7.22	.670	.399
8.12	.754	.428
8.93	.830	.434
9.86	.916	.422
10.67	.991	.426
11.60	1.077	.426
12.54	1.165	.427
13.40	1.244	.428
14.33	1.331	.429
15.46	1.437	.427
16.36	1.520	.428
17.30	1.608	.427
18.15	1.685	.430

TABLE VI

Forces on 4" Brass Cylinder

May 5, Temp. 27.5°C

VL		C
Ft²/Sec	Meter²/Sec	
7.85	.729	.425
8.67	.806	.426
9.72	.903	.430
11.20	1.040	.428
12.22	1.135	.434
13.64	1.266	.432
14.94	1.387	.435
15.97	1.484	.426
17.17	1.595	.433
18.78	1.744	.436
19.62	1.822	.435
20.99	1.949	.433
22.00	2.043	.434
22.93	2.130	.433
24.33	2.261	.434
26.82	2.492	.430
29.27	2.719	.430

TABLE VII

Forces on 4" Wood Cylinder

April 14, Temp. 19°C

VL		C
Ft^2/Sec	$Meter^2/Sec$	
7.55	.702	.438
8.49	.788	.425
9.74	.905	.421
11.12	1.033	.418
12.20	1.133	.419
13.47	1.251	.420
14.67	1.362	.421
15.85	1.472	.423
17.06	1.585	.421
18.30	1.700	.423
19.55	1.816	.424
20.68	1.919	.424
21.93	2.039	.425
23.19	2.151	.421
24.40	2.266	.422

TABLE VIII

Forces on 4 1/2" Wood Cylinder

April 14, Temp. 20°C

VL		C
Ft^2/Sec	$Meter^2/Sec$	
8.48	.788	.440
9.44	.877	.442
10.86	1.009	.429
12.37	1.149	.432
15.08	1.401	.431
16.41	1.525	.435
17.80	1.653	.432
19.16	1.780	.433
20.60	1.914	.429
21.94	2.039	.430
23.20	2.154	.427
24.61	2.288	.425
26.00	2.413	.421
27.40	2.546	.413

TABLE IX

Forces on 5" Wood Cylinder

April 7, Temp. 29°C

VL		C
Ft^2/Sec	$Meter^2/Sec$	
9.34	.868	.412
12.04	1.118	.412
15.50	1.440	.414
18.30	1.699	.416
21.32	1.982	.417
24.40	2.268	.413
27.46	2.550	.406
30.70	2.851	.370

TABLE X

Forces on 5 1/2" Wood Cylinder

April 21, Temp. 24°C

VL		C
Ft^2/Sec	$Meter^2/Sec$	
10.48	.973	.455
11.75	1.091	.423
13.23	1.229	.424
15.30	1.421	.428
17.04	1.582	.429
18.84	1.750	.430
20.98	1.948	.423
23.45	2.179	.420
25.80	2.397	.408
27.40	2.544	.398

TABLE XI

Forces on 6" Wood Cylinder

April 2, Temp. 23°C

VL		C
Ft^2/Sec	$Meter^2/Sec$	
11.21	1.042	.426
12.46	1.158	.416
14.05	1.305	.425
16.03	1.489	.429
17.59	1.633	.427
19.44	1.805	.428
21.36	1.984	.424
23.20	2.153	.422
25.62	2.381	.427
27.89	2.589	.419
30.27	2.810	.393

TABLE XII

Force on 1 and 2" Wood Cylinder. N.P.L. Balance

These are not plotted for reasons explained in paper. The measurements are on a second balance in another part of the tunnel.

1" Cylinder

VL		C
Ft^2/Sec	$Meter^2/Sec$	
1.85	.172	.547
2.08	.193	.541
2.59	.241	.558
3.02	.281	.554
3.65	.339	.554
4.17	.387	.551
4.50	.418	.567
5.12	.476	.553
5.70	.530	.560

TABLE XII, Continued

2" Cylinder

VL		C
Ft²/Sec	Meter²/Sec	
3.87	.359	.466
4.32	.401	.456
4.85	.451	.447
5.64	.524	.444
6.12	.568	.443
6.76	.628	.429
7.32	.680	.445
7.80	.724	.448
8.48	.788	.451
9.40	.874	.453
9.94	.924	.455
10.66	.990	.449
11.32	1.051	.449
12.25	1.139	.450

TABLE XIII

Integration of Pressure Measurements

Note—A square 5 small divisions on each side is used as an intermediate unit in measuring the areas.

Pressures are measured as heads of benzene on a slope of .18403 (i.e., vertical head = measured head x .18403). This unit varies with the temperature and allowance has been made in all computations.

Diam. of Cylinder	1	1	1
Static Plate Reading	1.35	4.32	10.32
Speed Derived from Static Plate Reading M.P.H.	16.36	29.37	45.28
Temperature	24.5°	18.0°	17.8°
Max. Positive Pressure on Front	2.20	6.55	16.14
Speed Derived from Max. Positive Pressure M.P.H.	16.85	29.20	45.90
Area of Curve Sq. Inches	32.92	47.51	38.30
Area of 100 Squares	Mean 35.08, variation about 1/5%		
Area of Curve for Back of Cylinder Only	27.93	38.54	31.32
Mean Decrease in Pressure on Back	2.00	5.50	13.41

TABLE XIII, Continued

Diam. of Cylinder	1	1	1
Ratio Max. Pressure on Front to Average Decrease on Back	1.10	1.19	1.20
Force Represented by one square (lbs)	.0005610	.000113	.000339
Area of Curve in Squares	93.9	135.6	109.2
Force per Unit Length in Lbs.	.000527	.0153	.0370
Total Force on Cylinder	.0948	.275	.665
Coefficient Calculated			
From Static Plate Velocity	.568	.510	.520
From Maximum Pressure Velocity	.536	.516	.506
Coefficient from Force Measurements	.62	.62	.62

TABLE XIII, Continued

Diam. of Cylinder	1	3	3
Static Plate Reading	20.00	10.96	4.57
Speed Derived from Static Plate Reading M.P.H.	63.0	46.6	30.1
Temperature	19.0°	26.9°	26.8°
Max. Positive Pressure on Front	31.90	17.10	7.06
Speed Derived from Max. Positive Pressure M.P.H.	64.40	46.95	30.20
Area of Curve Sq. Inches	50.22	47.90	41.20
Area of 100 Squares	Mean 35.08, variation about 1/5%		
Area of Curve for Back of Cylinder Only	42.60	34.47	30.30
Mean Decrease in Pressure on Back	30.35	9.84	4.31
Ratio Max. Pressure on Front to Average Decrease on Back	1.05	1.74	1.64
Force Represented by one Square (lbs)	.000565	.0003410	.000[illegible]826
Area of Curve in Squares	143.4	136.6	117.5

TABLE XIII, Continued

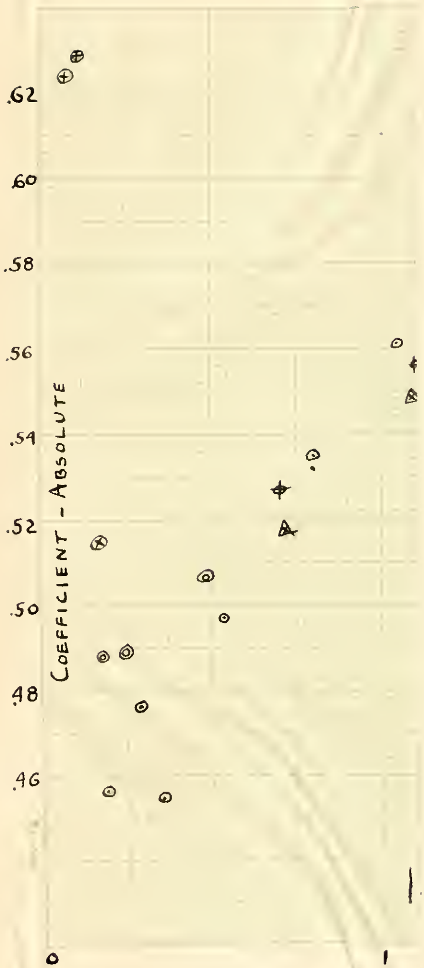
Diam. of Cylinder	1	3	3
Force per Unit Length in Lbs.	.0810	.0401	.0934
Total Force on Cylinder	1.456	1.718	1.674
Coefficient Calculated			
From Static Plate Velocity	.589	.415	.415
From Maximum Pressure Velocity	.564	.413	.400
Coefficient from Force Measurements	.62	.43	.43

TABLE XIII, Continued

Diam. of Cylinder	4 1/2	6
Static Plate Reading	4.83	4.37
Speed Derived from Static Plate Reading M.P.H.	30.97	29.41
Temperature	20.5°	21.3°
Max. Positive Pressure on Front	7.33	7.30
Speed Derived from Max. Positive Pressure M.P.H.	30.84	30.78
Area of Curve Sq. Inches	44.67	40.36
Area of 100 Squares	Mean 35.08, variation about 1/5%	
Area of Curve for Back of Cylinder Only	35.05	33.05
Mean Decrease in Pressure on Back	5.00	4.74
Ratio Max. Pressure on Front to Average Decrease on Back	1.47	1.540
Force Represented by one Square (lbs)	.000515	.000688
Area of Curve in Squares	127.4	115.1

TABLE XIII, Continued

Diam. of Cylinder	4 1/2	6
Force per Unit Length in Lbs.	.0656	.0792
Total Force on Cylinder	1.179	1.422
Coefficient Calculated		
From Static Plate Velocity	.430	.431
From Maximum Pressure Velocity	.453	.395
Coefficient from Force Measurements	.43	.43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.58
.56
.54
.52
.50
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COEFFICIENT - ABSOLUTE
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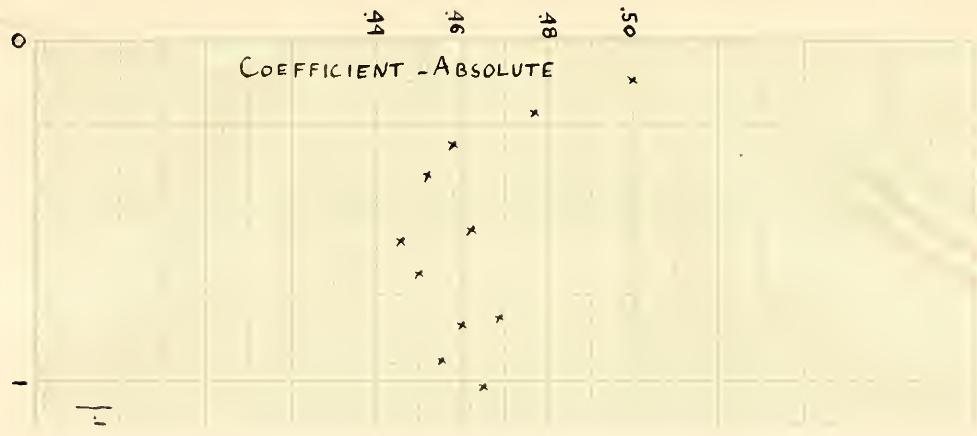

COEFFICIENT - ABSOLUTE
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